One and the same voice, when singing on *a* or *b*♭, would sometimes give the simple and sometimes the duplex form.

The following general propositions are, we think, established by these experiments :—

1. The generic character of ū from *d* upwards is given by the prominence of a single partial tone. Above *b*♭ this partial is the prime. Below *a* with certain voices it is the octave of the prime.

2. The amplitude of the second partial is sometimes as much as nine times greater than that of the prime, but the sound will be recognised as ū when the second partial is only three or four times the size of the prime, provided the pitch of the prime is below *a*.

3. Below *d* the experiments do not warrant positive conclusions. The prominence of a single partial is less marked, and at the same time the vowel quality of the spoken sound is very poor.

4. The average pitch of the reinforced tones is generally lower than the average pitch of the group for ō.

5. We have not detected any single note having the marked characteristics for ū that were possessed by the note *b′*♭ for ō.

6. There is a critical pitch in the neighbourhood of *a* or *b*♭, at which a sudden change takes place in the wave-forms given by certain voices in singing ū. Further experiments are desirable in this sound at this pitch. The critical pitch is not perfectly constant for a single voice.

As some doubt may not unnaturally be felt whether or not the singular change of character observed in ū at or near the note *a* may not have been due to some peculiarity of the instrument, we may repeat that, being fully aware of this danger, we tried the experiment repeatedly with changed mouth-pieces, changed vibrating discs, changed springs, and with different thicknesses of tin-foil, but invariably with the result that certain voices passed at this pitch from the simple to the duplex ū. It must not be forgotten, too, that one voice, using the same instrument, carried the simple ū as low as *f*.

7. This points to the fact that the change from simple to duplex ū is not made on account of the requirements of the ear, but on account of the difficulty in adjusting the mouth-cavity so as to continue the simple form on the lower notes. We have seen that, not only do different voices make the change at different pitches, but a single voice gives sometimes one and sometimes the other form at pitches near its critical point. We thus see that the simple and duplex forms overlap, and that in this case sounds of the same pitch, which differ by a whole octave in the pitch of their single prominent tone, are accepted by the ear as generically the same vowel.

The prominent tone in ū is found to lie in the region from *a* upwards, but we have not found any one sharply-defined pitch to be characteristic of ū.

With reference to the theory of characteristic tones in vowel-sounds we draw attention to the very great prominence of the tone *b′*♭ (the characteristic tone of ō) when voice 1 sang ū or *b*♭. What may be termed the average pitch in ū is for notes near *d* and *e* lower than that in ō, owing to the absence of the third partial; but it is noteworthy that, as the voice runs up the scale, the constituents of ū are on an average higher than those of ō for *g*, and in the case of voice 1 even for *b*♭; this arises from the comparative smallness of the prime. When the simple form is reached the average pitch of the constituents is lower than that for ō. If, instead of looking at the average pitch, we look at the pitch of the highest and most prominent constituent, we find that this pitch is identical for ū and ō when these vowels are sung on *g* by voices 1 and 5, and when sung on *b*♭ by voice 1.

Further light is thrown on the whole theory of vowels by experiments on the vowel ō as uttered by a mechanical contrivance made by Prof. Crum Brown and lent by him to us for investigation by help of the phonograph. It consisted of a bagpipe reed fitted to a tube leading into a gutta-percha resonance chamber which had such a form and such openings (tentatively arrived at) as caused it to speak a very good ō when the reed was blown. No one listening to this sound could doubt that the letter ō was being distinctly spoken. We held this apparatus to the phonograph, and the record obtained from it gave back a remarkably good ō, better indeed than the original, as the jarring noise of the reed was lost. When the pitch of the reed was changed, the same vowel continued to be given. The pitches in our experiments with this resonance bottle ranged from *e* to *e′*. We had thus a constant cavity producing unmistakably the same vowel throughout an octave.

Table VII. gives the constituents of each ō thus artificially produced. They agree with the ō's pronounced by the voice in being composed of a prime and its octave for notes above *b*, also in having the first three partials on *e* and *f*♯. On *g* the third is stronger than in the human voice. No specially strong reinforcement appears to exist in this artificial ō for *b′*♭, or any tone near it, but there is a wide range of reinforcement extending on both sides of this pitch. It may be stated that this artificial ō was what might be called a bright ō whose pitch of general or of maximum resonance might be expected to be somewhat higher than that of the ō's given by the human voice.

We draw attention to the fact that even supposing the prime not to have been reinforced at all, this irregular gutta-percha cavity must have reinforced tones, more or less strongly, through a range of at least fifteen semitones, namely, from *e′* to *g″*. This fact follows from the analyses, but it was confirmed by another and independent experiment. A short tube was inserted into the bottle in place of the reed and the end of the tube applied to the ear so that the cavity acted as a resonator to sounds from outside. The keys of a piano were then struck in succession and the listener noticed by the bumming noise in the bottle what tones were reinforced. On working down the scale resonance was first noticed at *g″*♯. It increased, and became excessively strong on *f″*, and remained nearly equally strong on *e″*. It then diminished a little, but became very intense again on *c″*♯, then diminished again, but even on *g′* and *f′*♯ it was much stronger than could be accounted for by the strengthening of the second partial. Below this the experiment was not suited to detect the influence of the cavity as a resonator on account of the reinforcement of upper partials in the notes struck. It sufficed, however, to prove that this irregular cavity possessed not only one, but probably several proper tones, so near to one another as to give the effect of a resonator strengthening, more or less, every tone between widely distant limits.

We have also made a few experiments on the vowel sounds "awe" and "ah," which we shall write *a*° and ā respectively. The phonograph spoke these sounds fairly well, though not quite so well as the sounds ō and ū. It may here be remarked that our phonograph was not capable of registering very high tones; a shrill whistle, however loud, produced no effect on the tinfoil. This fact proves, independently of the analyses, that in the case of those vowel sounds which it did speak well, the comparatively low partials were sufficient fully to characterise the vowel. The observations show that for *a*° the first three partials are prominent where ō has only two; and that for the same part of the scale ā has four consecutive partials all prominent. The fifth partial was considerably strong for ā when sung on *e*, where ō was composed of three partials and *a*° of four. Tables VIII. and IX. show the analyses made of these two vowels as sung by voice 5.

This communication is already so long that we must defer our general remarks to another number. FLEEMING JENKIN

J. A. EWING

EXPERIMENTS ON THE RELATIVE SPECIFIC GRAVITIES OF SOLID AND MELTED MATERIALS AT THE TEMPERATURE OF FUSION

DR. MUIRHEAD communicates the following account of experiments undertaken at his request, for the purpose of testing the notion that the earth's crust, as it cooled, became relatively heavier than the molten mass within; that the crust, breaking into fragments, sank; and this process, going on time after time, by and by built up a sort of honeycombed arrangement of the earth's interior :—

"*Railway Works, Leeds,* March 30, 1878

"DEAR SIR,—I have carefully gone over the experiments *re* the melting of metals in contact with liquid metals. I was certain on this point, from more than half a century's observation, before you wrote me, and I think I stated that conclusion in a former letter. I have now only to indicate the order in which I conducted the experiments, the result of which I now communicate.

"With several different compounds of brass, at various temperatures, I melted similar compounds; skimming the metal in the crucible, I laid the solid piece carefully on the clean surface, which piece, coating itself partially by chilling the liquid metal, very soon re-absorbed a sufficient amount of heat to be fused,

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and, fusing from the *bottom side*, gradually dissolved. I then placed similar blocks of metal endwise on, when, dipping beneath the surface, they bounded back to the surface, and subsequently dissolved, endway down. These results apply to various weights and sorts of compounds. I then conducted similar experiments with cast-iron, and found that the facts were still more conspicuous in the cast-iron (all of the same tendency) than in brass.

"Placing the iron on the surface of the liquid iron, a rapid chill set in, and a coating of iron, apparently about ⅛″ thick, attached itself to the cold iron, but very shortly re-melted, when the cold iron disappeared with it. I then dropped a small piece of cold iron (the same being dried to prevent explosion) endwise on to the surfaces of the liquid metal, when, bounding back to the surface, it melted in that position.

"The argument applies precisely to the experiments conducted in lead.

"In all cases the cold metals were relieved of any exterior ingredient by being well filed over. In every case of brass and iron the material melted was about 1″ in diameter and 4″ in length, each piece being round. With regard to the lead, the pieces varied in size, weight, and form, but all the experiments resulted in the same way. "JOSEPH WHITLEY"

"*Railway Works, Leeds*, April 11, 1878

"MY DEAR DR. MUIRHEAD,—Confirming my letter of yesterday, I have now to report the results of several experiments which, you will see, perfectly coincide with and demonstrate the truth I have again and again assured you of, viz., that all liquid matters that are susceptible of solidification will, when solid, float upon similar matter when in a liquid state.

"I intimated to you in my last that I feared I could not, in a small crucible, sufficiently fuse granite and whinstone, and in my experiments of yesterday, although I melted my crucible, I did not sufficiently liquefy the granite so as to float a piece upon the liquid mass. I therefore deferred further manipulations till to-day, and having secured a quantity of whinstone, I also determined to alter my course and to take advantage of a much larger focus of heat than that of a furnace 30″ × 20″ × 20″, with a 60-lb. crucible. So I called upon Messrs. Taylor Bros. and Co., ironmasters of this town, and with their permission I proceeded as follows:—

"Being passed over by their manager to a subordinate officer—a worthy and very intelligent fellow, and, by the way, a strong believer in the doctrine that matter sinks in like matter when melted—we went to a furnace where we had three tests with whinstone, which he said disappeared, and I believe that he was justified in the two first experiments, because he was not sufficiently up in his observation as to notice a stream of gas liberated from a bubble formed on the surface by the melting of the whinstone immediately under it. In the next furnace we went to we had a large quantity of liquid 'cinder' 'tapped out' of a furnace into a trough. I really wish you could have seen it; to me the sight was grand, the gases given off by the melting of the whinstone blazed with a sort of blending of tints of purple, yellow, and green. I never saw anything so fine in flame. The whinstone was like a thing of life, so buoyant—of course the specific gravities of the liquid and solid materials varied considerably, and hence the buoyancy of the whinstone. We then tried a large number of small pieces of cinder—same as the liquid mass before us; but my friend the officer insisted that they went to the bottom—they certainly, except in one instance, never returned to the surface, because they liquefied before they had time to rise. My whinstone being done, and seeing that the results were not so satisfactory to my friend, I remarked that I was prepared to go all day and all night rather than give up the task of convincing him that his conclusions were wrong. I therefore suggested we should take a larger furnace, and deal with larger masses. We therefore, instead of dealing with quantities of 8 oz. weight, and weights of 1 lb., took pieces of 5 and 6 lbs. weight, each of solid cinder. Had my faith not been implicit I might have been deceived, for No. 1, 2, and 3 pieces went to the bottom, and my friend said, 'Now are you satisfied?' and I replied 'No, I am not.' Imagine his astonishment when No. 1 came bounding to the surface, and floated about like a cork, when the mass of heat had dissolved the coating which it clothed itself in at entering the bath and had begun to melt the original piece, up came No. 2 and 3, and I let him float them about on the surface with an iron rabble, so as to sear, as it were, the lesson sufficiently deep into his soul that it might never be erased. There were eight or nine jolly fellows looking on, and who enjoyed the joke, when my friend took off his hat and bowing politely said, 'Well, I am exceedingly obliged for the lesson you have taught me, and I shall never forget that all solid matter floats upon like matter when melted, as ice floats upon water.' Of course I had a joke and told him he was only one of a few who believed in the doctrine, and that he was the last convert. "JOSEPH WHITLEY"

P.S.—In the name of science I take this opportunity of tendering to Messrs. Taylor Bros., of the Clarence Iron Works, Leeds, my grateful thanks for their generous acquiescence in my request, and the facilities they kindly rendered in the experiments.—J. W.

THE INFLUENCE OF LIGHT UPON BIOPLASM[1]

SOME twelve months ago we briefly recorded in NATURE the results of our observations on the effect of sunlight on bacteria, and other organisms commonly associated with putrefaction and decay. Most of the experiments were subsequently described in detail in a paper communicated to the Royal Society. The chief of our earlier conclusions may be summed up shortly as follows:—

1. Light is inimical to, and under favourable conditions may wholly prevent, the development of these organisms, its action on the common forms of bacteria being apparently more powerful and rapid than upon the mycelial fungi which are prone to appear in cultivation-fluids.
2. This action appears to attain its maximum in the waves of greatest refrangibility. It is demonstrable in yellow light, but towards the red end of the spectrum sinks to a minimum.
3. The fitness of the cultivation-fluid to act as a nidus is not impaired by the insolation.

We found, moreover, that tubes containing a cultivation fluid and plugged with cotton wool, when removed to a dark place after exposure to the sun for a sufficient period remained perfectly clear and free from organisms for months. We thought, therefore, that the "germs" in these solutions had been completely destroyed by the solar rays.

While, however, we believe that, if the insolation be sufficiently prolonged, all the germs or spores originally present may be killed, and that, as regards bacteria, the insolation, under favourable conditions, need not be of very long duration, we have reason to think that, by *cell-walled* organisms, the destructive action of light may be resisted for a considerable period, and that the first result is to reduce the spore to a state of torpidity in which it may lie dormant for many months.

The investigation of this point, however, must necessarily extend over a long time; and in the above remarks we would wish to be understood as offering a suggestion rather than a definite conclusion.

We noticed last year that sunlight had no retarding effect on the *action* of the "indirect ferments," or, at least, of the soluble ferment of yeast (*zymase* of Béchamp, *ferment inversive* of Berthelot), which we used for our experiment. More recently, however, we have tested the effect of prolonged insolation on the soluble ferment itself, and have found that, at the end of three weeks' exposure to a midsummer sun, the *zymase* had entirely lost its characteristic property of hydrating cane-sugar, while a corresponding specimen, which had been kept in the dark, still retained its energy. It would appear, therefore, that the action of light affords no means of distinction between the "organised" and the "indirect" ferments.

We have attempted to elucidate the intimate nature of this action of light upon the organisms which have formed the subject of our experiments, and we have evidence pointing strongly, as we think, to the solution of the problem. Early in 1877 we set ourselves to this task, and, in order to obtain some insight into the effect of light upon certain organic bodies, we made a number of observations upon oxalic acid.

We have elsewhere[2] shown that a solution containing 0·63 per cent. (decinormal) is entirely decomposed by a somewhat prolonged exposure to strong sunlight when air is present. We now find that in a corresponding solution, *in vacuo*, no change

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[1] By Arthur Downes, M.D., and T. P. Blunt, M.A. Oxon.

[2] *Chemical News*.

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